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Electrical Science in Foreign Journals.

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences, Vol. lxxvii., No. 16.

On the Advantages of the Circular Compass.—By M. Duchemin.—According to the author the principle advantages of the circular compass are: 1. Double the magnetic power, for a determined diameter, of that of a needle whose length would be equal to that diameter. 2. The existence of two neutral points instead of one, thus maintaining constant the position of the two poles. 3. A better suspension of the magnet, which is mounted on an agate cup. 4. Augmentation of the sensibility of the compass proportional to its diameter. 5. The possibility of equilibrating the magnetisation by means of a second magnetised circle, modifying the position of the first by a calculated quantity, and permitting of the compensation of the compass before the departure of the vessel. The last modification was suggested to the author by Capt. Dumas Vence.

No. 17.

*Action of the Condenser on Induction Currents.**—By M. Lecoq de Boisbaudran.—Physicists generally admit, I think, that the spectral modifications produced by the introduction of a Leyden jar into the induced circuit are the consequence of the variations of temperature, and not of the physical nature of the discharge. Permit me to present some remarks in support of that opinion:—I. We observe the thermic superiority of the condensed spark to the ordinary spark, by comparing the two spectra obtained under the different conditions with the same substances; for the bands suffer a gradual modification in one direction by the successive employment of (1) vapour or gas; (2) the ordinary induction spark drawn from the surface of a solution; (3) the same spark drawn from a solid substance or ignited to fusion; (4) finally, the condensed spark. As: (1) the orange band of lithium, faintly visible in the gas flame, becomes superior to the red band with the ordinary induction spark drawn from a concentrated lithic solution (*Comptes Rendus*, 19th May, 1873, p. 1264), and is stronger, relatively to the red band, when the same spark starts from the solid salt. (2) With an ordinary induction spark, and a solution of chloride of tin, the bands Sn 558·9, and Sn 556·1 are feeble; with the same spark drawn from the solid tin-salt, these two bands are rather strong; with the condenser they are very brilliant. II. The effects of the condenser being due to the augmentation of temperature, there is a gradual change from the spectra obtained with the brush to those with the ordinary spark, and to those that result from the use of a powerful Leyden jar. III. The action of the condenser does not appear to be the same on different spectra; for example, the lead bands, Pb 560·7, Pb 438·6, Pb 424·5, &c., are strongly reinforced, while the bands of the second order of air appear to possess only a moderate relative intensity. IV. The several bands of the same spectra are not always equally affected by the condenser. V. The bands, considerably strengthened by the condenser, become nebulous and of a certain size. VI. The enlargement of the narrow bands at a high temperature is explained by the perturbation suffered by the molecular movement when the forces applied are very considerable. VII. The bands of emission of solid or liquid bodies are nebulous. VIII. (Entirely relating to spectral analysis). IX. The sparks, from various induction coils, present spectral differences due to inequalities of the developed temperatures. Thus, certain *small* coils give naturally, with saline solutions, spectra analogous to those which are obtained with large coils, when aided by means of a condenser, or drawing the spark from the solid salt. I have estimated, contrarily to the opinion of some, that in order to obtain with small coils the *spectral effects* comparable with those of large coils, it is necessary to increase the length and resistance of the induced circuit. M. Gaiffe has constructed two coils of moderate size (19 centimetres), one with a large induced wire, the other with a wire four times longer and a seventh of the section. The effects from the latter are similar to those from my large Ruhmkorff coil, while from the former the effects are similar to those of a small coil.

* See also "Comptes Rendus," lxxiii., p. 943 et seq.

The Journal of the Franklin Institute. Vol. lxvi., No. 4.

Lightning and Lightning Rods.—A Letter from Mr. David Brooks.—My attention has been called to an article of Mr. Mott's on "Lightning and Lightning Rods," published in the August and September numbers of the journal, which contains many theories and notions at variance with those published by myself about a year since. Mr. Mott says: "Thus his Majesty's ship *Dictator* was struck and damaged by lightning at Martinique, in the year 1794, during fine weather and a clear sky." When effects are produced without cause miracles are wrought, and this is the latest recorded instance of a miracle. It is difficult to understand how any one who possesses any familiarity with the laws governing the propagation of the electric current, can believe that such an occurrence ever took place. "The return stroke often kills men and animals, or destroys buildings which are at the time at a great distance from the place where the direct discharge occurs." I have no hesitation in emphatically asserting that there is no such thing as a "return stroke" in an electrical discharge. There is a general inductive disturbance, or re-combination, whenever disruptive discharge takes place; but it is simultaneous with the discharge, and not an after result, and never "kills men and animals, or destroys buildings which are at the time a great distance from the place where the direct discharge occurs." In speaking of the effect and virtue of points over knobs or balls, my experiments show the very opposite of what he states to be true, and consequently I can neither appreciate nor subscribe to his remark or conclusion that, "Hence it (a point) may prevent a disruptive discharge which might otherwise occur." On the evening of the 4th of July, 1872, a thunder-storm passed over this city. Within the space of an hour over one hundred buildings were struck by lightning, or the effects of the lightning were noticed in over a hundred different places, leaving out many, probably a greater number, where the effect was unnoticed, or no damage produced. The discharges or flashes of lightning occurred as often as once per second during a portion of this hour. There were at least five thousand pointed rods in this city, and how much did they "produce an escaping current," or "prevent the accumulation of electricity," or neutralise the charged cloud?" Probably they did their level best, and prevented a discharge occurring oftener than once per second. The above example affords a practical illustration of the ability of points to silently discharge the accumulated electricity of the clouds, the earth, or the "return stroke," or the whole combined. Mr. Mott says "points are of vital importance before the instant of discharge arrives," and, he might add, of

no account when they occur. "An improper termination of a lightning-rod is to connect it with the gas or water pipes of a city or village beneath the surface of the earth. Theoretically, it is the best possible termination; practically, one of the very worst," &c. Mr. Mott may connect a rod to earth in his way, and throw in any desired amount of charcoal. I will connect the same rod to a gas or water pipe, and then apply a current to the rod, and demonstrate that this current goes to the pipes in at least the proportion of a thousand to one. The author, further on, refers to Mr. Crosse's experiments, whereby he discharged fifty Leyden jars twenty times a minute, accompanied by reports "as loud as those of a cannon." This experiment was probably performed about the time the *Dictator* was struck, "during fine weather and a clear sky." I will only remark, in conclusion, that it would have been more satisfactory had the author adopted the phraseology of many parts of my paper without interpolations.

On the Molecular Changes Produced in Iron by Variations of Temperature.—By Prof. Thurston.—The conclusions drawn by Prof. Thurston as to the molecular changes produced in iron by variations in temperature will not be uninteresting to the telegraph engineer. It seems very probable investigation will be found to confirm the deductions from experiments, and will justify the following conclusions:—That the number and nature of those molecular forces which determine the physical condition of matter are not yet fully ascertained, but that these forces manifest themselves in, at least, three distinct modes of action, and, as thus exhibited, they are known as repulsion, cohesion, and polarity. That the force of repulsion is, apparently, heat motion, or some closely related phase of energy; that the force of cohesion bears some resemblance to that of gravitation, but seems not to be identical with the latter, and that the force of molecular polarity, which determines the molecular relations of position, seems to bear some distant resemblance to that of magnetic polarity. That the law which governs the variations in intensity of these forces with changes of intermolecular distances, is undetermined, and that it has not been expressed by any mathematical formula, except approximately and for a limited range. That the magnitudes of the intermolecular spaces, and, consequently, the volume of any mass, are variable with changes in the relative magnitudes of the forces of cohesion and repulsion. That the resistance offered to change of form is determined by the relations, in intensity, of the forces of polarity, and of those forces which determine intermolecular distances. That, at the "absolute zero" (−461·2° Fahr.), cohesion and, consequently, the strength of the material, have their maximum value, heat energy having disappeared. That, at very high temperatures, heat energy exerts a separating force between particles, which entirely overcomes the other forces, and matter assuming the gaseous state requires the action of extraneous force to preserve its volume unchanged. That, at intermediate points, matter in either the solid or the liquid state exhibits a definite degree of separation of molecules, which is determined by the intensity of the repulsion due to heat motion, a position of equilibrium being assumed which, with the same substance, is invariable for the same temperature. The application of some kind of force is required to disturb this equilibrium and to produce this change of volume. The amount of this force is determined, for any given extent of disturbance, by the maximum value of cohesion for the substance and the quantity of heat which has been required to raise it from the absolute zero of temperature. The sum of the applied force, and of the force consequent upon the presence of heat motion, must exceed cohesive force to produce dilatation, while this cohesive force, added to the externally applied force, must exceed the force of repulsion to produce diminution of volume. That the distinction between the solid and liquid states of matter is due to the action, in the former, of the force of polarity, which gives stability of form, while in the latter this force is extremely feeble, and disappears altogether before the boundary line between the liquid and gaseous states is reached. That combined stability and elasticity of volume may be produced by the equilibrium of attractive and repulsive forces, but that stability and elasticity of form demand the coexistence of cohesion and polarity. That the general effect of increase or decrease of temperature is, in solid bodies, to decrease or increase their power of resistance to rupture, or to change of form, and their capability of sustaining "dead" loads. That the general effect of change of temperature is to produce change of ductility, and, consequently, change of resilience and power of resisting shocks and of carrying "live" loads. This change is opposite in direction and, usually, greater in degree, than the variation simultaneously occurring in tenacity. That marked exceptions to this general law have been noted, but that it seems invariably the fact that wherever an exception is observed in the effect upon tenacity, an exception also may be detected in the effect upon resilience. Those causes which produce increase of strength appear always to cause a simultaneous decrease of ductility, and *vice versa*. That experiments upon copper, so far as they have been carried, indicate that, with that metal, the general law holds good. That iron exhibits marked deviations from the law, between ordinary temperatures and a point somewhere between 500° and 600° Fahr., the strength increasing between these limits to the extent of about 15 per cent., with good iron. That this variation becomes more marked and the observed effects are more irregular as the metal is more impure. That, above 600°, and, at temperatures below 70° Fahr., the general law holds good with iron, its tenacity increasing with diminishing temperature below the latter point, at the rate of from about 0·02 per cent. to 0·03 per cent., for each degree Fahrenheit, while its resilience decreases in a much higher but not well determined ratio for good iron, and to the extent of reduction to one-third its ordinary value or less, at 10° Fahr., when "cold short," and, in the latter case, the set before fracture may be less than one-fourth that noted at a temperature of 84° Fahr. That the viscosity, ductility, and resilience of metals are determined by identical conditions, and that the fracture of iron at low temperatures has, accordingly, been found to be characteristic of a brittle material, while, at higher temperatures, it exhibits the appearance peculiar to ductile and somewhat viscous substances. The metal breaks, in the first case, with slight permanent set and a short, granular fracture, and in the latter with, frequently, a considerable set and the form of fracture indicating great ductility. The variation in the behaviour of iron, as it approaches the welding heat, illustrates the latter condition in the most complete manner. That the precise action of the elements with which iron is liable to be contaminated, and the extent to which they modify its behaviour under varying temperatures, remain to be fully investigated, but that the presence of phosphorus, and of other substances producing "cold-shortness," exaggerates to a great degree the effects of low temperature in causing loss of toughness and resilience. That the modifications of the general law with other metals than iron and copper, and in the case of alloys, have not been studied, and are entirely unknown. That these conclusions are sustained by experiments of both physicists and engineers. The practical result of the whole investigation is that iron and copper, and probably other metals, do not lose their power of sustaining

"dead" loads at low temperatures, but that they do lose, to a very serious extent, their power of sustaining shocks or resisting sharp blows, and that the factor of safety in structures need not be increased in the former case, where exposure to severe cold is apprehended, but that machinery, rails, and other constructions which are to resist shocks should have large factors of safety, and should be most carefully protected, if possible, from extremes of temperature. It will be noticed that nothing in the evidence here quoted indicates crystallisation or any change of molecular grouping to be consequent upon simple change of temperature.

The Telegrapher. Vol. ix., No. 377.

Electricity at the Cincinnati Industrial Exposition.—Inserted in last number.

No. 378.

Relation of Changes in the Length of the Earth's Radius Vector to Frequency of Auroras.—(Abstract of paper read by Prof. E. B. Elliott, of the Treasury Department, at the recent meeting of the American Association for the Advancement of Science). That auroras are of scientific origin is admitted by all who are conversant with the subject. The exhibition of extensive and brilliant auroras is invariably attended with all the other well-known evidences of magnetic storms, such as the corresponding rush to and fro of electric currents in telegraphic conductors, to such an extent as to seriously disturb and interrupt the ordinary transaction of business upon the lines, and the irregular and extraordinary deflections of the magnetic needle. Whenever the magnetic condition of the earth is disturbed, corresponding disturbance of the magnetic needle, disturbances of electric condition of telegraphic conductors, and manifestation of auroras invariably result. The question had suggested itself whether, among the many causes which may produce disturbance in the electric or magnetic condition of the earth, and its invariably attendant magnetic and auroral storms, might not be ranked the annual or periodical lengthening and shortening of the earth's radius vector. If the year be divided into twelve equal parts it will be found that the length of the radius vector (or the distance of the earth from the sun) augments most rapidly during that portion of time which corresponds to the month of March, and diminishes most rapidly during that portion which corresponds to the month of October; the date, however, of most rapid diminution being during the latter part of September, *i.e.*, September 25. The following table gives the increment and decrement of the logarithms of the length of the radius vector for the monthly periods above referred to:—

TABLE SHOWING THE CHANGES IN THE LENGTH OF THE EARTH'S RADIUS VECTOR.

Increments and decrements of logarithms of the length of the earth's radius vector, by months, or periods of 30½ days each, as deduced from the American Nautical Almanac for the year 1868:—

Months or Periods.		Logarithms.
January (Jan. 1.0 to Feb. 0.5)	†	·00100
February (Feb. 0.5 to March 1.0)	†	·00276
March (March 2.0 to April 1.5)	†	·00369
April (April 1.5 to May 2.0)	†	·00360
May (May 2.0 to June 1.5)	†	·00258
June (June 1.5 to July 2.0)	†	·00092
July (July 2.0 to Aug. 1.5)	—	·00096
August (Aug. 1.5 to Sept. 1.0)	—	·00261
September (1.0 to Oct. 1.5)	—	·00361
October (Oct. 1.5 to Nov. 1.0)	—	·00368
November (Nov. 1 to Dec. 1.5)	—	·00273
December (Dec. 1.5 to Jan. 1.0)	—	·00098

† Denotes increments.
— Denotes decrements.

In a general catalogue of auroras, classified by years and months, compiled by Professor Joseph Lovering, of Cambridge, Mass, and published by the American Academy of Arts and Sciences, may be found the following table, showing the distribution by calendar months of 12,263 auroras observed:—

January	1134
February	1169
March	1436
April	1170
May	713
June	455
July	544
August	838
September	1219
October	1341
November	1154
December	1090
Total number of auroras observed	12263

From this table it will be seen that the months of maximum frequency of auroras are the calendar months of March and October. The greater frequency of auroras observed during the months of northern winter, when the earth is near its perihelion, as compared with the number at midsummer, when the earth is near its aphelion, is doubtless attributable, in large part at least, to the superior opportunities for observation afforded to northern observers by the greater length of the winter nights as compared with those of summer, the observations having been exclusively made in the northern hemisphere. The marked coincidence between the frequency of auroras and the rates of approach and recession of the earth to and from the sun was deemed worthy of note, not alone for its independent interest, but also on account of its possible bearing on the discussions now in progress relative to the coma of comets, the zodiacal light, and certain other cosmical phenomena.

No. 379.

A Phenomenon that Cannot be Explained.—In support of a statement concerning the storm of January last, and how it was possible that the Northern Pacific line could be worked with a good ground wire on in the centre, Mr. C. H. Haskins says—I only know the fact. I would be very glad to explain it satisfactorily to myself if I could. And to show that the statements that I made were exactly true, I enclose a letter from Major George H. Smith, an old and experienced telegrapher, and Mr. L. Ashley, the operator at Hobart Station, where the ground wire was on. Major Smith's statement is endorsed by Superintendent O. C. Greene, who investigated the phenomena at the time, and reported them to me. Supt. Greene and Major Smith are too well known as reliable gentlemen to need any endorsement from me. Major Smith says:—"The point set forth by Mr. Haskins, that in order to work the wires through it became necessary to put on an intermediate earth to carry off or counteract the atmospheric electricity, is a fact that can be attested by several telegraphers. It occurred on the circuit (N. P. R. R.) between Brainerd, Minn., and Fargo D. T. (a distance of 138 miles), during the electrical storm of January, 1873). It being impossible to work through on account of the heavy and frequent electric waves, I asked Hobart, a station near the centre of the circuit, and whom I raised with difficulty, to put on his 'ground' and try to do some repeating for us. He had no sooner done so than I found Muskoda, a station near Fargo, calling me, and broke him without difficulty, and continued to work with offices west of Hobart's ground wire for several hours, when, the storm abating, the ground commenced being felt, and was finally removed. I will state that this ground wire of Hobart's was *perfect;* and as the phenomena was a

great surprise to me and others, it was thoroughly examined and tested, the line refusing to work through without its use. There is no mistake about the matter; I studied the working of the wires during the storm carefully, and formed my theory in regard to it at the time and on the spot; but the purpose of this letter is simply to sustain Mr. Haskins's assertion of the fact in question." Mr. Ashley says: "I will give you, as near as I can recollect, the facts concerning the working of this line through my ground wire, during the severe snow-storm of last January. No. 2 wire had been broken west of this office the first day of the storm, which, I think, was on the 7th inst., and two section men were sent out to repair it. It being at the top of the pole, above the railroad wire No. 1, I gave them particular instructions to leave No. 2 so it would not touch No. 1 and cause a cross. The next morning both wires worked very badly, and it was supposed they were crossed when No. 2 had been repaired the day before. It was also thought to be crossed east of here. Some time during the morning I put my ground wire (which is a good one) on No. 1 west, to see if the line worked clear east of here. It instantly began to work perfectly clear. Some office was working, and to my surprise they signed "Be," the call for Muskoda, thirty-nine miles west of me. Brainerd, eighty-one miles east, answered him, and they continued their conversation a few moments, at the close of which I called Brainerd, and reported the case to Mr. Smith, who was at the key. He called up other offices west of me and conversed with them, my ground wire remaining on during the time, the line working clear and perfect. When I would take off the ground the line worked as though very badly crossed; when the ground was replaced it worked clear again, east and west. Mr. Smith said it was something he could not see into—that he would have to study it out—and directed me to leave my ground wire on, as long as the line worked better with it on than it did with it off, which I did, keeping it on during the greater portion of the day."

The Resistance of Semi-conducting Substances.—By F. L. Pope.—The fact of variation in the apparent resistance of imperfect joints in telegraph wire, under different battery powers, to which Mr. I. N. Miller was the first to call attention, so far as I know, is a matter deserving careful investigation. A paragraph in Fleeming Jenkin's recently published work on "Electricity and Magnetism," gives the clue to the cause of the phenomenon, I think. He says: "The resistance of a film of dirt does not appear to follow Ohm's law. When the potential of the charged and insulated conductor is increased, the loss by 'creeping' increases in a much lighter ratio; probably the conduction is partly due to numerous small discharges from one speck of dirt into its neighbour." This also would go to show that in working a badly insulated line the loss of current by escape would be increased in a greater ratio than the increase of battery power—a fact which observing telegraphers have long suspected, but which, as far as I am aware, has never been distinctly proved. Acting on this hint, I made a single test on 20 glass insulators on a wet day. A test with two cups gave nearly twice as much apparent resistance that a test with 20 cups did. It is very unwise to jump at conclusions from a single experiment, and I merely mention it in order to direct the attention of electricians to the matter, which is of much practical importance.

Reale Istituto Lombardo di Scienze e Lettre Rendiconti. Vol. xiii, 1873.

On Electrical Adherence. By P. Cantoni.

Wiener Academische Anzeiger. No. 17, 1873.

On Some Applications of Air Friction in Measuring Instruments.—M. Topler.—In most fine instruments in which a swinging mass gradually comes to rest, through the deadening influence of air friction, the action of such friction has been little studied as a means of stilling the mass. The author has found, from various experiments, that the influence may be turned to good account for this purpose. The deadening action is most marked where the moving system oscillates in a hollow space of such form that the enclosed air is compelled to share in the motion, with the greatest possible friction. The following is a description of a regulable air-deadening for magnetic bars, mirror galvanometers, torsion balance, &c.:—Under the thread-suspended magnet, conceive a fixed cylindrical case with plane top and bottom. In it is a light, four-cornered plate of aluminium mica, stiff paper, or the like (of such size that it nearly fills the vertical section of the case), connected with the magnet by a thin rod passing through a hole in the cover. The plate has, like the magnet, the S N direction. If the system now oscillates, the deadening is but small. The body of air moved with the plate only affects the moment of inertia of the system. But a very strong deadening may be obtained if two cross walls be introduced through vertical slits in the cylinder wall, at east and west, and their inner edges neared to the middle of the plate. Magnetic bars may thus be so deadened that in deflection-experiments they show no oscillation. The degree of deadening can be modified at will, according as the cross walls are pushed in little or far. The effect is easily explained; for the cylindrical space is divided by the plate and the cross walls into four quadrants; and when the north end of the needle swings, *e.g.*, to the east, the air is forced to pass out of two quadrants north-east, and south-west into the quadrants north-west and south-east, and it must stream through the slit-intervals at the edges of the plate and the cross walls with great friction. An example may here be given. A small magnetometer had a good cylindrical deadening case of 95 m.m. diameter and 53 m.m. height. The magnet and belongings weighed about 7 grammes. The deadening plate hung about 7 m.m. from the case and the cross walls. For eight successive oscillations the following proportion-numbers and corresponding logarithmic decrements were obtained:—

Oscillations.	Log. decrement.
607	
382	0·201
241	0·200
151	0·202
95	0·201
60	0·200
38	0·199
24	0·200

When the cross walls were quite removed, the decrement was only 0·044, and, the case having next been removed, 0·029. When the deadening-plate was taken so large that its edges were only about 3 m.m. from the walls, the magnet came to rest after one oscillation. The log. decrement, it will be seen, is quite constant, although in the above case the arc of oscillation decreased in the proportion 25 : 1. Hence this air-deadening in galvanometers is of similar value with the known metallic deadening. For small galvanometers a combination of air-deadening with the deadening of metallic masses is to be recommended. The apparatus above described is very suitable for measurements on air-friction. It may also be hermetically sealed, and arranged as an air-thermometer, and then, dipped in hot water, it affords a means of studying the dependence of air-friction on the temperature. It may also be filled with other gases. The author further describes an application of the principle to cathetometer observations; a small mirror, swinging in a narrow case (where, through air-friction, its motion is as if in a viscid liquid) being observed by a telescope.

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences, Vol. lxxvii., No. 18.

Mutual Action of Voltaic Currents.—By M. Bertrand.—A portion of a controversy with M. Helmholtz.

The Best Dimensions of Electro-Magnets.—By M. the Count du Moncel. This paper will be translated in full in an early number.

No. 19.

Examination of the Law proposed by M. Helmholtz to represent the Action of two Current Elements.—By M. J. Bertrand.—This law, very different from that of Ampère, has been deduced from calculation alone. I have to show the inexactitude of the new formulæ proposed by him (M. Helmholtz), which do not even accord with the hypothesis he deduces from them. In analysing the several forces which exist between two portions of a current, M. Helmholtz admits a repulsive one, independent of the distance.

On Magnetism.—By M. J. M. Gaugain.—I have still to make known a certain number of results of experiments which have relation to latent or dissimulated magnetism; but it appears to me indispensable to present some observations on the modifications to which the magnetism of an iron horse-shoe magnet is subject when we detach the armature. The elder Hæcker, I believe, has remarked that when a bar of steel is magnetised by an iron horse-shoe magnet, it is in an unstable magnetic condition, which is modified each time we attach or detach the armature; but that twenty repetitions of this operation produces a magnetic condition of the bar no longer subject to change, even when the bar remains without its armature for a whole year. These observations apply to soft iron without restriction; we cannot communicate sensible magnetism to a soft iron electro-magnet when we do not apply the armature, even if the inducing current be very intense; whilst, on the contrary, when the armature is applied the iron is strongly magnetised, even by the current, from a single Daniell cell. When the armature, detached for the first time, is applied, and again detached without re-establishment of the inducing current, the magnetism of the iron is much enfeebled; and this can be easily measured by the method of induction currents previously described. I can describe other experiments in which detaching the armature appeared to produce increase of magnetism. I have passed through the coils of an electro-magnet, provided with its armature, a current of determined intensity; I have interrupted this current, and I have detached the armature; subsequently I have applied and detached the armature a very great number of times, in order to obtain the constant magnetic state, and I have then determined the value of the current developed by this constant magnetism. The increase does not depend upon the greater time of magnetisation. The increase no longer occurs when the inducing current has been several times interrupted without detaching the armature.

On Several Cases of Intermittence of the Voltaic Current.—By M. Casin.—In pursuing some researches that I have undertaken on the heat of electro-magnets, I had occasion to observe several cases of intermittence of the voltaic current, not, I believe, hitherto noticed. First experiment:—A voltaic circuit is formed of 20 moderate-sized Bunsen elements, and a coil of 960 turns, wound on a tube of iron of 8 centimetres diameter, and about 1 millimetre in thickness. The circuit can be closed or opened at will by the aid of a platinum point dipping into mercury, each communicating respectively with the reophores. While the platinum does not touch the mercury, and while we put them in communication with the armatures of a plate-glass condenser (of 3 square metres surface) a distinct vibration continues in the iron core. The same effect is produced when, suppressing the condenser, we interpose a layer of alcohol between the mercury and the platinum point. The roaring ceases when we suppress the alcohol, and leaves only a layer of air, also, when the point is dipped into the mercury. These facts indicate that the current, in the first case, passes across the glass, across the alcohol in the second, and that its *passage is intermittent*. The tube of iron submitted to a rapid succession of magnetisations and demagnetisations occasions the feeble roaring sound heard in the coil. A galvanometer indicates only a continuous current, as would be expected when the intermittence is so rapid. I think the cause of the intermittence to be the condensing action of the glass and the alcohol. When the two faces of the insulating substance have acquired a certain electric potential, a discharge takes place across the insulating layer; the magnetism of the core increases during the charge, and decreases during the discharge. The sound is produced during the diminution of the magnetism. Another experiment:—The platinum point is mounted in a fixed hammer. The mercury and point communicate respectively with the armatures of a condenser of 1 square metre surface. The rest of the circuit is arranged as before. The point being plunged into the mercury, it is gradually raised until the spark just crosses the alcohol. There the point remains fixed, a succession of sparks is established, and continues for some time. The mercury evidently oscillates beneath the point. The possible cause of this oscillation is that the spark is formed by the vapour of mercury. The elastic force of this vapour depresses the level of the liquid. This purely mechanical cause may not be the sole one. The facts would appear to show that *the interposition of a sufficient resistance in a voltaic circuit determines the intermittence of the current.* The laws of this intermittence must be studied with the condenser, in order that the periods may be sufficiently long for observation. We should not neglect to draw the possible inference that a *current is a succession of changes which are accomplished periodically in the circuit.*

Poggendorff's Annalen der Physik und Chemie. No. 7.

Reaction of Secondary Currents in an Unchanged Circuit on the Primary Current of a Leyden Battery.—By M. Riess.—The secondary current has a weakening action on the primary. The reaction of a series of secondary currents decreasing in strength because of the conveying wire being changed, undergoes two phases. In the first (commencing with perfect conduction of secondary current without sensible weakening of the primary), the action increases, and so the primary currents decrease in strength; in the second, the reaction decreases, hence the primary currents become stronger. If we take two secondary currents in the first phase, the weaker corresponds to a weaker primary; if in the second, to a stronger. The latter effect is not remarkable, and the former may perhaps be explained by the weaker secondary having a longer duration, and so obtaining a firmer hold (so to speak) on the primary. For long, there was no other known way of altering the secondary current produced in a secondary wire at given distance from a given portion of the primary than that of altering the wire which closed the secondary circuit. The experiments were made with different secondary wires. Other methods are now known. Thus, suppose, besides the secondary circuit, a second (auxiliary) wire, whose ends may be either connected or free. When they are connected, the secondary current is stronger than when they are free. Greater changes of the secondary current may thus be obtained if the wire used for closing the secondary circuit is not drawn out, but bent in the form of a narrow N. The best way, however, is to arrange the wire spirally (the auxiliary wire having this form also). M. Riess experimented with the arrangement

consisting of a spiral of copper wire ($53\frac{7}{12}$ feet long, $\frac{1}{3}$ line thick) placed in the circuit of a battery of three Leyden jars, and having opposite to it a secondary spiral, which is connected by copper wire with a third copper spiral. This spiral is distant 1 line from another (auxiliary) spiral, the ends of which can be joined by 1 foot of copper wire. Galvanometers are used to measure the currents. The following numbers will indicate the effects obtained (the secondary circuit was at first open, and there was therefore no secondary current):—

	Secondary Current.	Primary Current.	
		0·900	100
Auxiliary spiral closed ..	0·58	0·570	63
Auxiliary spiral open ..	0·31	0·765	85

Here we find that while the opening of the auxiliary spiral brought down the secondary current in the proportion 58 to 31, the primary rose from 63 to 85. The spirals were next removed, and platinum spirals of various lengths were introduced into the secondary circuit. It was then found that when the secondary current was thus reduced (through lengthening of its wire) from 0·61 to 0·32, the principal current was *reduced* in the proportion 54 to 30. It is thus shown that two nearly equal pairs of secondary currents, obtained in different ways, may react quite oppositely on the primary current; in the first set of experiments the weaker secondary corresponds to the *stronger* primary; in the second set, it corresponds to the *weaker*. Without using any auxiliary spiral, secondary currents of different strength may yet be got in an unaltered secondary circuit, if merely the unexcited portion of this is placed in various ways. Thus the U form of the conducting wire gives the strongest secondary current, the straight form a weaker, and the N form the weakest. Here, also, the case occurred of the weakest primary current corresponding to the strongest secondary, and the strongest primary to the weakest secondary. The most interesting way of making secondary currents of different strength in the same wire react on a primary current is that of producing the change of the secondary current by a second secondary current acting in different directions on the first. The secondary current is weakened if the current acting on it has the same direction, and strengthened in the opposite case. The apparatus here remains unaltered in the different reactions, whereas in the first series of experiments, a part of the apparatus, the auxiliary spiral is made inoperative through opening. A number of different experiments were made with other arrangements. They all appeared to support the view that the secondary current requires a certain time in order to be able to act on the primary, and that the action increases up to a certain limit with the prolongation of this time. Thus, *e.g.*, in one series of experiments, the galvanometer was taken out of the secondary circuit, so that both secondary circuits were equal, and contained only copper wire. Here the secondary current was stronger than in the preceding cases; yet in strengthening or weakening it (by altering the direction of the current) its reaction was weaker than in the preceding cases. We have to suppose that it had too little time, and the much weaker currents in the previous series had a much greater reaction on the primary (when they were weakened or strengthened) because of their longer continuance. If the strengthening and weakening of a secondary current, through change of its circuit, may be explained by the lengthening or shortening of its time of existence, it is not so easy to explain its strengthening and weakening in the unaltered circuit. The remarkable two phases of reaction of secondary currents, which have their circuits altered, do not here occur. The strongest secondary current diminishes the primary most, and, whatever two secondary currents are taken, it is to the weaker of them the greater current corresponds. The reason of this is obvious; the stronger secondary current is produced by a more powerful excitement; and contains, therefore, a greater quantity of electricity, which acts on a longer piece of the primary circuit; whence its greater action on the primary current. By other experiments it has been shown, that with increasing quantity of electricity of the secondary circuit, its reaction on the primary increases, even if the reacting piece of the secondary circuit remains constant. The portion of the wire circuit excited by the primary current remaining unchanged, the circuit was opened at a distant point, and the two wire ends were connected with the surfaces of a condenser. The more extensive these surfaces are, the greater is the quantity of electricity set in motion in the wire circuit, and the weaker appears the primary current. Should we argue from these earlier experiments to the cause of the new ones, we should be led to the supposition, that through action at a distance on one piece of the secondary circuit the quantity of electricity circulating in the secondary current could be changed. The quantity of electricity excited in a piece of the secondary circuit, lying on the primary, might everywhere remain the same, but a smaller or greater portion of it, according to the arrangement of apparatus, might complete the circuit course, while the remaining portion might go back the same way as it had come on excitement. This is conceivable; unfortunately it cannot be tested by magnetic deflection. The above described new kind of reaction of secondary currents on the primary current of a Leyden battery, may be thus simply stated:—A wire circuit, part of which is excited through a given battery current remaining unaltered, different secondary currents, excited in it successively, act on the primary current in such a way that the weaker secondary current corresponds to the stronger primary. The reaction varies thus in the same direction, with the strength of the secondary current. By altering the circuit of wire excited by the primary current, secondary currents may be obtained of the same value with those in constant circuit, and these may, as has been long known, alter the primary in the opposite direction, so that the weaker primary current corresponds to the weaker secondary. The new fact is, in any case, important. If the quantity of electricity in the differently obtained equal secondary currents is different, it appears that in a current, with the same excitation, the quantity of electricity may be altered, through action at a distance. If, on the other hand, the quantity of electricity in these currents is the same, we learn the unexpected fact, that currents with equal quantities of electricity, which equally heat the circuit through which they flow, may yet react on the primary current in opposite directions.

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On the Various Modes of Electrical Testing.—By Professor Cantoni.—The professor here also treats of hypothesis in electrostatics; and raises objections to certain passages in Dr. Tyndall's work referring to the existence of two fluids.

We congratulate the University of Edinburgh, says *Nature*, on being the first in the United Kingdom to recognise the duty of universities so to frame their regulations for degrees in science as to encourage original work in opposition to mere book-knowledge. The University of Edinburgh has just issued a regulation that every candidate for the degree of Doctor of Science shall in future be required to submit a Thesis containing some original research on the subject of his intended examination, and that such thesis shall be approved before the candidate is allowed to proceed to examination.